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Thoughts in Fusion Energy Development

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Thoughts on Fusion Energy Development

**On the Occasion of the Fusion Power Associates Annual Meeting and Symposium,
3-4 December 2008**

R. F. Post

First off, I wish to thank Steve Dean and his Fusion Power Associates for honoring John Nuckolls and me, and for giving us this opportunity to comment on a field or research that has been our passion for decades. In my case, I would also like to thank Ken Fowler for proposing the theme of the symposium to Steve Dean many months ago and then diligently following through on its details..

In what I have to say I will be talking about paths to fusion and about fusion's history as I recall it. Not about the negative aspects of history, as in "those who forget history are doomed to repeat it," but the positive view that: "If we remember that in the past we had a clearer vision of the path to fusion, and if we have gotten off that path, we know that the path exists and that we can find it again if we try."

Where to begin? - And what to highlight about the six-decades-long love affair that I have had with fusion research? My fascination with fusion really began early in 1952 as a result of three classified lectures given by Herb York. I was then a year out of graduate school and working at the Radiation Laboratory (now LBL). Herb's series of lectures covered the physics issues of controlled thermonuclear reactions (CTR) and described the U. S. fusion programs at Princeton University, headed by Lyman Spitzer, and at Los Alamos, headed by Jim Tuck. Both groups were working on versions of the only game in town at that time - trying to use specially shaped magnetic fields to stably contain a 100 million-degree-hot ionized gas - plasma - composed of electrons and fusion fuel nuclei - heavy hydrogen isotopes

For the benefit of the non-scientists, getting power from a magnetically confined fusion plasma is the nuclear equivalent of burning natural gas in a furnace - except that here the furnace liner is to be made up of non-material magnetic field lines. The other main present approach to fusion - using lasers to heat a tiny pellet of fusion fuel to ignition - did not exist. The laser had not yet been invented and John Nuckoll's pioneering work in the area of laser-based fusion research was yet to come.

Herb York's lectures on magnetic fusion had a specific goal in mind - to stimulate the interest of us physicists to join him in forming a new laboratory on a site near Livermore. This new lab was to have fusion research as one of its main goals.

To make a long story short, after Herb's lectures there was ferment among many of us - trying to think of ways to solve the controlled fusion problem. Several of us then joined the new lab, some to work on controlled fusion and others to work on classified military applications.

At this point I think it is important to make clear the underlying source of our fascination with fusion research – then and now. Even before 1952 it was beginning to be evident that within perhaps less than a century the world could no longer count on fossil fuels for its ever-increasing energy demands. In long term it would have to rely on energy released in nuclear reactions, that is, either fission or fusion. To those of us that went to Livermore with Herb it seemed obvious that the fusion of heavy hydrogen was the way to go, and we pointed to the world's huge fusion fuel reserve - the fact that 1 in every 6500 atoms of hydrogen in water was a deuterium atom. Here was a fuel reserve that was not only virtually inexhaustible, but one that would be cheap and universally available; no fusion OPECs and no future conflicts born of competition for limited fuel resources.

To emphasize the significance of fusion's fuel reserves, here is a thought experiment: Think about the amount of ordinary water – H_2O - that would flow through a city water main about a foot and half in diameter at normal pressures . Then think about putting that flow of water into a deuterium separation plant, using well-known energy-efficient separation techniques. From that input of ordinary water there would come out of the separation plant a small stream of heavy hydrogen - deuterium. This deuterium, if distributed to fusion power plants and fused to completion, would represent a fuel energy input rate equal to the entire world's energy input rate today –all the oil and natural gas wells, all the coal mines, all the hydroelectric plants – everything! And as to inexhaustibility, how long do you think it would take to pump all the water in the oceans through an 18-inch water main?

A bit more fusion history: Serious effort on magnetic fusion research began in about 1950 in classified research programs in the U. K., the U. S. and the Soviet Union. By 1955 it was apparent that magnetic confinement of a hot plasma was a much more complex process than first thought, so that at the 1958 Geneva “Atoms for Peace” Conference, these three countries declassified and described all of their fusion research results in order that the fusion quest could be pursued by all the nations.

To achieve net fusion power it is necessary to heat and then to confine a fusion plasma long enough for the fusion energy released to exceed the energy required to heat the fuel to fusion temperatures. However, as of Geneva 1958 it was clear that the plasmas in every magnetic configuration that had been tried exhibited plasma instability and turbulence, leading to unacceptably rapid loss of the plasma. This universal observation – of the negative effects of turbulence on magnetic confinement - defined the central problem for magnetic fusion research from that day forward, up to and including today.

First, some basics of the magnetic confinement for the non-scientists: Strong magnetic fields change the straight-line orbits of the ions and electrons of a plasma into tight spirals moving along the field lines. This inhibits escape of the particles across the field lines, but unless something is done about it, does not restrict their motion along the field lines. In magnetic fusion research, the choice of what that “something” should be has from its beginning separated magnetic fusion researchers into two distinct groups–

those solving the “problem of the ends” by using “closed-field” systems - field lines chasing their tails inside a doughnut-shaped chamber, or those studying “open-field” systems, that is, using a tube-shaped bundle of field lines and then “plugging” the end leaks by strengthening the field at the ends to form “magnetic mirrors.” But the plasma physics issues introduced by making one or the other of these choices are profoundly different, and (here comes the personal bias) the choice that was actually made, in the late 1980s, by most of the world’s fusion programs - to restrict their research to closed-field systems - has severely slowed our progress toward the fusion goal.

Up to the mid-1980s the world’s magnetic fusion energy program was on the right path. The program was a broadly based one, with sizeable experiments investigating a variety of both closed and open systems, backed up by an extensive theoretical and computational effort. But, not surprisingly, the criterion that was adopted by the policy makers at that time for judging the merit of one approach over another was how close the magic fusion numbers – plasma confinement time, plasma density, and plasma temperature - that had been achieved experimentally came to the numbers required for net fusion power. By the middle 1980s one closed-field system – the tokamak - was the clear winner by this criterion. Why? Because early on, starting with experiments by its Russian inventors, it was found that all you needed to do to get better numbers out of a tokamak was to build a bigger one. Though the tokamak was very difficult to analyze theoretically, and was clearly plagued by a variety of plasma instabilities, nevertheless when one plotted the confinement times of succeeding generations of ever-larger tokamaks against the square of their plasma radius the data lay on an upward-sloping straight line – aiming directly at plasma fusion ignition in some future, necessarily very large, tokamak.

As I see fusion’s history, this simple curve sounded the death knell for all approaches that did not resemble or support the tokamak in some way – specifically, it virtually terminated the study of open-ended, systems, apart from some pockets of resistance at Tsukuba in Japan and at Novosibirsk in Russia. This shift in program breadth happened even though great progress had been made in open-ended mirror systems following the invention of the tandem mirror in 1976 by Ken Fowler and Grant Logan, here at the Laboratory, simultaneously with its invention in Novosibirsk, Russia, by Dimov. In that heyday for mirror research a large tandem mirror experiment here at Livermore, TMX, was proposed and construction was completed in 18 months. Tandem mirror systems were also built with similar speed - at MIT and the University of Wisconsin in the US, and at Tsukuba in Japan. At Livermore TMX was followed by an upgrade – TMXU, and then by the construction of a really large tandem mirror, MFTX. Days after its completion and first shakedown tests MFTX was mothballed and all mirror-based work in the U. S. was terminated.

Where are we in magnetic fusion research today? We are partway down a long trail that dates back to 1985, when a proposal for a really large, internationally sponsored, tokamak – ITER - was made. It then took 20 years – until 2006 - before funding agreements (10 billion dollars) and a site was chosen by the international partners. Another 10 years will be required for construction and 20 years of operation are planned,

after which a “demo” tokamak, one actually generating electricity would be considered (since the ITER experiment will generate only heat).

To wrap up (here comes the personal bias): Can we afford to wait that long for fusion? Are there a faster, better, ways to get there? Here I’ll be discussing magnetic fusion only. I’ll not talk about the impressive progress in laser-based fusion towards fusion ignition. – John Nuckolls will certainly be covering that work.

First, about ITER: I give ITER high marks for keeping magnetic fusion from dying on the vine, for the international cooperation it has fostered, and for the fusion-related science and technology that was developed and is being developed to implement it. But ITER is like the TV ads for a new wonder drug: “If you are patient this drug will do wonders, but – look out for those side effects!” The “side-effects” of ITER, in my opinion, have been catastrophic for magnetic fusion research. They include: (1) narrowing a program that cries out for breadth to insure success, (2) turning away bright young researchers from magnetic fusion research because its course is already a “done deal,” and (3) drying up funding for anything that does not support or at least resemble a tokamak.

Enough of negativity! I would like very much to finish this talk on a positive note. First, our critical need for clean and sustainable sources of energy represents a real opportunity for fusion research if we take advantage of it. One way to put the situation today is to talk of it in terms of “present reality” and “future reality.” “Present reality” says: “In the present economic climate and with our prior commitments there is no way we can support a new effort.” The prime example of “future reality” was when John F. Kennedy said “we are going to put a man on the moon in ten years.” He knew that the science and rocket technology needed for a moon landing was there, along with the money to pay for it. I believe that we are in a similar situation today with respect to the magnetic approach to fusion power. We have the basic scientific understanding, the computational horsepower, and the technology to take a new, broader, look at the problem. And we certainly have the financial wherewithal. For example, we are spending 700 billion dollars a year to import oil. One week of that rate of expenditure – 11 billion dollars - is equal to the entire U S. magnetic fusion funding over its 56 plus years of existence. A 4/10th percent tax on that oil could pay for a fusion budget that is a factor of ten larger than the present budget.

Are there better, faster-to-develop, approaches to magnetic fusion than the tokamak? Yes, there are! As an example, I would cite the recent findings of a DOE-sponsored committee that is taking a new look at open-ended systems – in particular at new forms of the tandem mirror that we call “ATMs” (for “Axisymmetric Tandem Mirror” – not for machines for getting money – yet). The committee is chaired by a former Lab employee and mirror group leader Tom Simonen (who is doing a great job). Its members include several Lab employees and retirees, plus researchers from other labs, including MIT, Princeton, the University of Texas, and Los Alamos. We are now writing the final report. It concludes that the open-ended ATM represents a simpler and easier-to-engineer, approach to magnetic fusion than ITER, since it is modular in nature and,

being axisymmetric, it employs only simple circular coils to create its confining magnetic fields.

What is even more important is that we believe that the ATM could be free of the plasma turbulence that haunts the tokamak and that dictates its huge size. In support of this possibility is a plasma stabilization concept analyzed theoretically by Lab physicist Dmitri Ryutov (when he was at Novosibirsk in the 1980s). His theory has been confirmed in detail by a series of experiments in the Gas Dynamic Trap axisymmetric mirror machine at Novosibirsk. In the GDT a hot, dense, plasma is confined stably for times in agreement with theoretical predictions, and the plasma shows no evidence of turbulence.

Do I think that the ATM could be a "future reality"? Yes I do! Do I think that it is the only worthwhile new approach to magnetic fusion? Definitely Not! Do I think this country should rapidly re-invigorate its magnetic fusion program? You bet I do!

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